Philosophy meets history in the discovery of weak neutral currents

By: Samuel Schindler

Hardly any episode in the history of modern science has received as much philosophical attention as the discovery of weak neutral currents in 1973/4. This paper will revisit this episode, criticise the philosophical views about signal-noise discrimination that have been made on the basis of it, and advance a new proposal as to how neutral currents were actually discovered.

Galison (1983) published the first historical account of the discovery, which described in great detail the complex events that preceded the discovery in the collaborations of “Gargamelle” at CERN and “HPWF” at the NAL in Batavia (what later became known as Fermilab). Neutral currents, prior to 1970, were thought to be effectively inexistent and physicists had not much interest in them. Although in the late 1960s Steven Weinberg devised an model unifying electromagnetic and weak interactions that required the existence of neutral currents, the physics community remained unmoved until 1970 when Gerard ‘t Hooft showed that the model was renormalisable and thus not merely a mathematical construct but physically meaningful. Now, physicists (among them the collaborations at CERN and Batavia) started to take very seriously the possibility of neutral currents existing and modified their extant bubble and spark chambers in order to detect neutral currents in neutrino-nucleon scattering. Yet, the detection of neutral currents didn’t come cheap. The major problem physicists struggled with was “noise” caused by “neutron background” (in bubble chambers) and unidentified muon events (in spark chambers). Since both noise sources “mimicked” neutral currents (i.e. they were experimentally indistinguishable from neutral currents), any discovery claim about neutral currents had to make sure that the noise was not wrongly identified as the actual “signal”. Noise thus had to be estimated by means of Monto Carlo computer simulations. Many arguments in- and outside the Gargamelle and HPWF collaborations ensued about the adequacy of those estimates before and after both groups published their findings in 1973 and 1974 respectively. Nevertheless, neutral currents were accepted as a real phenomenon sometime around 1974. Up until this point, there is no disagreement about the relevant events in the historical literature. Yet, the actual reasons for acceptance of neutral currents as real are strongly disputed.

Pickering (1984) suggested that neutral currents were a socially constructed phenomenon, i.e. neutral currents were accepted as real because they were “socially desirable” to both theoretical and experimental physicists. Pickering also claimed that already the bubble and spark chambers of the 1960s were capable of producing and actually did produce a neutral current “signal”. Yet, due to lack of interest within the community, this signal was simply ignored and subsumed under background noise without further consideration. This has been disputed by Miller and Bullock (1994), who hold that the in particular the bubble chambers of the 1960s at CERN were simply too short to for arguments (to do with the spatial distribution of events along the chamber) to be made which eventually led to the acceptance of neutral currents in the 1970s. In this paper I will revisit this dispute and show that the argument from spatial distribution was far from being a “proof beyond doubt” (as Miller and Bullock call it). In fact, it too was severely undermined by neutron background. Yet, I do not embrace a sociological explanation of the acceptance of neutral currents as real for reasons to be explicated below.

Philosophers, too, became interested in the discovery of neutral currents with the seminal “Saving the Phenomena” by Bogen and Woodward (1988). Bogen and Woodward distinguish between data and phenomena, whereby (typically unobservable) phenomena are inferred from (typically observable) data, and theories are said to explain and predict the former but not the latter. Bogen and Woodward cited bubble chamber photographs as a major example for their concept of data and neutral currents as an instance of a phenomenon. According to them, data to phenomena inferences are drawn in scientific practice when one establish that the inference base, i.e. the data is reliable. Bogen and Woodward assign no role to higher level theory in this process. This view I want to contest on the basis of the historical case (see below).

In a similar "empiricist" vein, Mayo (1994) has tried to appropriate the discovery to support her account of "severe testing". According to Mayo, scientists regularly distinguish physical effects from noise by embarking on testing hypotheses that state either that the measured data can or cannot be attributed to background effects. If either hypothesis passes a test that it is unlikely to pass if it were false, then the data, accordingly, are to be attributed to background noise or not. For this sort of test to be meaningful, however, the data already have to be reliable. But this was by no means a settled question. Particularly the scepticism surrounding the Monte Carlo estimates of background noise, which was pivotal to any neutral current discovery claim (see above), seems to be underestimated by Mayo.

Contrary to Galison, Bogen, Woodward, and Mayo, I hold that the experimental evidence for neutral currents remained undermined by neutron background in 1974 and even beyond. The curiosity of Monte Carlo simulations of background events being highly contested before 1974 but not afterwards remains unexplained by those authors. It can be shown that Monte Carlo estimates, even during the discovery years, were highly flexible and controversial.

Why then were neutral currents accepted as real? I suggest that they were because the theory postulating them, i.e. the Salam-Weinberg model, gave physicists non-empirical reason for belief in their existence in the form of the simplest mechanism for unifying electromagnetic and weak interactions. I buttress this claim by discussing (i) phenomenological alternatives to the SW model that were just as compatible with the data but which were largely neglected by the physics community, (ii) electroweak alternative models that did not postulate neutral currents but which, due to the postulation of several unobserved heavy leptons, were ontologically much more costly than the SW model, and (iii) electroweak alternatives that did postulate neutral currents, but whose Higgs mechanism for unifying electromagnetic and weak interactions was more complicated than the SW model. The discussion of (i)-(iii) will also show why sociological constructivism is not sustainable in this case: if the acceptance of neutral currents as real was due the desire to maintain the social cohesion of the profession, then it remains unexplained why none of the alternatives (i)-(iii) was pursued.